Solid Earth Discuss.,
https://doi.org/10.5194/se-2020-15-RC1, 2020

Interactive comment on "Fracture attribute scaling and connectivity in the Devonian Orcadian Basin with implications for geologically equivalent sub-surface fractured reservoirs" *by* Anna M. Dichiarante et al.

Anonymous Referee #1

Received and published: 28 February 2020

General comments

The topics of fracture attributes, scaling, and the relevance of outcrop fractures to fractured reservoirs are all of current interest. Systematic measurements of apertures and lengths are valuable contributions to the literature. The paper content is appropriate for the special issue and for the journal. The paper is mostly clearly written and is well illustrated. I believe that the technical content of this MS is for the most part interesting, valid, and defensible. But there are several areas where in my opinion improvement is needed.

The main claim of the paper is that it is possible to combine patterns for all types of fractures (opening-mode fractures and faults) imaged from micro-scale to regional scale to find meaningful size scaling patterns. Another claim is that such broad scale scaling observations can be used by making projections to other scales of interest to get input for or to provide information for 'realistic' reservoir models and as input for fluid flow simulations, etc.

There is room for improvement in how clearly these claims are stated, how they are related to previous work, and how well they are defended and supported.

Specific comments

1. The Introduction should have a clearer statement of claims. The key paragraph from lines 65-82 is mostly an inventory of the approaches used and some comments on curve fitting methods best moved to the discussion. The paragraph should be broken up to separate the inventory part from a revised and augmented section that explicitly spells out the claims; text that could start out 'Here we show that. . .'. A clear statement of the claims is essential. These claims also need to match the Conclusions. Neither of these conditions are currently met. The text doesn't make the claims clear. And the first conclusion (line 640) that the outcropping rocks are a 'direct analogue' is not a conclusion at all. This point was merely asserted in the text without much back up. The comments in line 604-605 seem to point just to a similarity in spacing values. But if this is a major conclusion it needs to be signaled more clearly and the evidence needs to be presented more effectively. It may be easier to just assume that the outcrops may be pretty good analogs and present the evidence for this without making it a major conclusion (but explain what you are doing).

2. The discussion of previous work is not adequate. The account needs to be more complete and more nuanced. Considerable work has been conducted on measuring size scaling of opening-mode fractures and faults and using relationships to project to unsampled (or inadequately sampled) scales for various purposes, including acquiring

data that can be used for getting input for flow models. Some of the relevant references are cited, but the scope of this previous work is not clear from the presentation. A more informed and complete account is needed. Also missing are some of findings from previous work that bear on the main claims of this MS. For example, large aperture size distribution data sets for opening-mode fractures have been collected from a wide range of sandstones (for example, Hooker et al. 2014) with the aim of predicting the average spacing/intensity of open fractures in reservoirs, and some of these predictions have been tested with horizontal cores or outcrop analogs. This previous work needs to be accounted for more explicitly. And providing a comparison of the results in the current study to the findings of Hooker et al. (2014) seems like an obvious step for putting the current work into context. It should also be addressed in the paper that extensive size-scaling investigations show that some sandstones do not show wide fracture size scaling ranges (see next item).

3. The limitations of scaling that have been found need to be acknowledged. Some tests like Hooker et al., 2009 show that, of example, microfracture aperture size distributions can be projected over several orders of magnitude to accurately predict intensity at sizes where fractures can impact production. But other studies, for example Laubach et al. 2016, show that in some sandstones, fractures have a narrow (characteristic) aperture size range and accurate projections from populations of small aperture sizes to large are impossible. This raises a concern that is directly related to the claims of the MS, since these observations imply that some fracture patterns do not scale (they don't have scale invariant properties; they can't be projected to or from larger or smaller sizes). What about circumstances where there is evidence of narrow fracture attribute size ranges? The evidence of the literature seems to be saying that some fractures patterns 'scale' but others do not. Taking a for instance from within the area represented by MS figure 3, Laubach et al. 2014 J. Struct. Geol. showed that two adjacent sandstones, influenced by faults or the same population as described in this MS, have drastically differing fracture attributes (size, spacing, porosity preservation). According to the proposition in this MS, these attributes should be predictable by the

MS's regional scaling relation. The test case should be discussed. It's hard to see how the regional could get this right, since these contrasting patterns are on the same scale. But the differences between the two sandstones are just those that would affect reservoir behavior. The MS claims on this topic need to be reconsidered or at least more completely explored in this light.

4. A related problem is in the description of studies that examine fractures having a wide range of sizes. The contrast between 'given scales' versus 'multiscale' is problematic, since 'given scale' seems to imply a narrow size range, but some of the studies cited under 'given scale' cover three or four orders of magnitude in scale. Maybe this is just an oversight. The types of structures analyzed and the size ranges analyzed need to be accurately portrayed. Moreover, since the outcrop structures in the reservoir sandstone analog in this MS seem mainly to be opening-mode fractures, the MS should pay closer attention to the previous work on scaling of opening-mode fractures in sandstone. It's surprising that there is no explicit comparison with the compendium of data in Hooker et al. (2014) for example. Or any discussion of the problems with collecting reproducible length data in sandstones outlined by Ortega and Marrett (which is in the reference list).

5. Is the distinction in this MS between 'given scales' versus 'multiscale' between data sets where the structures are clearly genetically related and of the same type, versus mixed populations of opening-mode fractures and faults that may not be related? The text I think could be read this way although this isn't stated explicitly. This part of the MS may be the most problematic. As noted in the comments below keyed to lines in the text, it is not always clear what kind of structure is being compared or projected. This needs to be corrected. Partly this problem in the text comes from using the general term 'fracture' to mean either opening-mode fracture or fault. This usage is stated right at the outset. But it leads to problems, confusing and obscuring the argument. The case is being made in the MS seemingly that, for example, patterns of faults visible on seismic can be used to predict the size distributions and connectivity of opening-mode

fractures at the reservoir/outcrop scale. This is a very considerable claim (I'm dubious). But the claim should at least be made explicitly and defended openly.

6. The claim that multiscale analysis can be useful for informing geological models has been supported by examples from the literature (these should be noted) but the claim that, for example, regional lineaments and seismically detected fault trace patterns can be used to predict meaningful fracture attributes at the grid block or smaller scale seems to me to be a bridge too far. If this is the claim, then a more convincing case is needed to support it. An obvious concern is the projections in figure 8. This figure seems to be saying that aperture and length can be predicted to within two orders of magnitude. What are the error bars on that already really wide prediction? How could such a prediction be used? The authors need to explain how to be useful, 'predictions' can span orders of magnitude (compare the prediction of Hooker et al. 2009 with the two orders of magnitude of size range in the projections of figure 8). Core and outcrop analog data show that fracture patterns at the core and outcrop scale can vary considerably in ways that directly impact fluid flow. As noted above, with adequate samples where microfracture populations are present some of these attributes can be accurately projected over three or four orders of magnitude to predict the attributes of large fractures. But these are cases where the small and large fractures are growing and interacting together in a specific rock type. Contemporaneous, interacting fractures are the ones likely to develop power-law size distributions (Cladouhos, Marrett 1996).

7. The referencing of certain points needs to be made more complete or more accurate. I've flagged instances in the following detailed notes. As it stands now, I don't think the MS properly represents or credits previous work.

8. There are a number of places in the text where reorganization is needed. The Introduction could be clearer. Some of the material in the Discussion looks more like observations/results. I've flagged some of these issues in the detailed line comments that follow. Improving the overall presentation will increase the impact of the paper.

9. I've flagged some areas in the text where meaning is unclear. For the most part the language seems fluent and precise. So, overall substantial conclusions are reached. But in its current form the MS could do a better job supporting the interpretations and conclusions.

Specific comments keyed to lines in the text and technical corrections

34 Lumping faults and opening-mode fractures together for analysis is a mistake in my opinion. This broad statement about fault and opening-mode fracture size scaling is true to an extent. Marrett et al. (1999) documented power-law scaling across 3 to almost 5 orders of magnitude regardless of rock type or movement mode. This was the study that established that such systematic relations exist and that extrapolation from one scale to some other scale of interest was a feasible approach. It's a surprising omission to leave this paper out.

One thing that Marrett et al. did not do was to mix opening-mode fracture and fault data sets. Doing so requires some defending. It's ok to make the general point that some faults and some opening-mode fracture populations show scaling patterns (although subsequent work shows that some populations do not scale in this way). But it is problematic to lump them all together as 'fractures' if in your description and discussion you let the reader lose track of which kind of structure you are talking about. You are making the claim that it doesn't matter which type of 'fracture' is analyzedâĂŤthat's fine if you can defend itâĂŤbut it's not convincing if you just use the all-purpose word 'fracture' in a way that makes it hard for the reader to assess the strength of your claim. For example, in lines (602-612) it's hard to tell which type of structure you mean.

Marrett, R., Ortega, O. J., & Kelsey, C. M. (1999). Extent of power-law scaling for natural fractures in rock. Geology, 27(9), 799-802.

34-37 Consider breaking this initial sentence up into parts. It packs together a lot of claims: a broad definition of 'fractures'; fractures of various types exist over a wide range of sizes; fractures control fluid flow and strength of crustal rocks; fractures influence the behavior of (some) oil and gas reservoirs. That's also a lot of ground to be covered by the three references you call out. But to many readers it may not be apparent to which of the above points each of the three references refers.

One way to revise this would be to cover the most general and less controversial topics about the importance of fractures first, followed by your definitions of geometric and spatial attributes. Then introduce size scaling and spatial arrangement studies systematically (the second clause in your sentence 1). Currently this part of the Introduction seems jumbled.

37 The line starting 'The heterogeneous distribution…' puts forth claims that seems like they ought to have some support from examples in the literature. And limiting the interest to 'in reservoirs' seems overly restrictive, since many of the examples of real concern for these matters comes from waste disposal, sequestration, and the like. Consider de Dreuzy et al. J. Geophys. Res., 2012. 38 How many readers will pick up on what you mean here by 'scaling parameters'? Maybe move into a more compact paragraph about size scaling.

41 'Schultz';

By 'in isolation' do you mean in disseminated arrays distant from folds and faults? The opening-mode fractures in such arrays can be closely spaced, and 'isolation' seems like a strange way of depicting that.

42 It's a dubious proposition that fractures sensu lato as you say can be described with an 'aperture' value. What does this mean for a fault?

43 And, for fractures at depth in the earth, by their chemical/cement attributes. I think this point ought to be mentioned. As described in a recent Reviews of Geophysics paper, if you just rely on geometric and spatial attributes, one's ability to interpret fractures is seriously restricted, detrimentally impacting the ability to discriminate fracture origins, determine whether or not outcrops are suitable analogs, assess fluid flow and

much else. I recommend noting this and calling out the reference where these aspects are explicitly discussed: Laubach, S.E., Lander, R.H., Criscenti, L.J., et al., 2019. The role of chemistry in fracture pattern development and opportunities to advance interpretations of geological materials. Reviews of Geophysics, 57 (3), 1065-1111. doi:10.1029/2019RG000671

44 Does this reference cover clustering (reviewed in a recent J. Struct. Geol. Special issue)? This list seems to have narrow referencing to cover this wide range of topics. All of these attributes have been treated in depth in the literature prior to the 2015 reference that you cite. And that document is not a review paper.

Also: clarify what you mean by 'continuity'. It's possible to have long (continues) open or sealed fractures, or continuous faults that are both seals and conduits locally. The trace continuity (or connectivity) of lines on an outcrop map or seismic section are no guarantee they represent continuity to flow. Also: see Philip et al. (2005, SPE REE) it is also possible in porous host rocks for discontinuous or disconnected fractures to markedly enhance permeability.

48 This is misleading. Wellbores and cores provide high-resolution sampling of opening-mode fractures and faults, but the 'fractures' in seismic data are (probably usually) faults. This paragraph gives the impression that the patterns of opening-mode fractures can be discerned on seismic. This has yet to be demonstrated; such fractures are mostly (maybe entirely) below seismic resolution.

You should be explicit about your assumptions. You assume that opening-mode fractures and faults are part of the same population. Presumably if they share scaling patterns if they were growing contemporaneously and interacting (like growth and attachment leading to length scaling, Cladouhos and Marrett 1996). Is there any evidence of this (apart from the scaling data)? The faults and the opening-mode fractures may be genetically unrelated to each other.

50 One of the challenges with opening-mode fractures is that individually they are

‘small’ with respect to the attributes that might make them visible on seismic. But they may not be small in other respects. For example, fluid flow. So I think you need to be more careful in this section where you are portraying scales. Because opening-mode fractures commonly have narrow widths and because lengths measured in outcrop are frequently short (in many cases because they are censored by outcrop size) some industry accounts say such fractures are ‘small’ and can be ignored (Stephenson and Coflin 2015); but some outcrop studies (and some tracer tests) demonstrate that individual opening-mode fractures can be as much as 500 m long (Laubach et al. 2016) and the tracer tests suggest some may be considerably larger (longer). Such long fractures are by no means ‘small’. The key to fracture permeability enhancement in a non-permeable host rock is the connected open fracture pathway (e.g., Long and Witherspoon, 1985 JGR; Philip et al. 2005 SPE REE) which is unrelated to visibility on seismic. A long, bed-confined opening-mode fracture might be a more significant feature with respect to fluid flow than a seismically visible fault (the size of the fault may not be the same as its size as a fluid conduit). Philip et al. (2005, SPE REE) showed that very narrow fractures can have pronounced effects on flow.

Stephenson, B. & Coflin, K. 2015. Guidelines for the handling of natural fractures and faults in hydraulically stimulated resource plays. Society of Petroleum Engineers. doi:10.2118/175910-MS.

Laubach, S.E., Fall, A., Copley, L.K., Marrett, R., Wilkins, S., 2016. Fracture porosity creation and persistence in a basement-involved Laramide fold, Upper Cretaceous Frontier Formation, Green River Basin, U.S.A. Geological Magazine 153 (5/6), 887-910. doi:10.1017/S0016756816000157

52 ‘networks’?

54-63 I think this paragraph needs some clarification. Is this mostly about how outcrops have been used, or about the extent that analyses have investigated fractures over a wide scale range? The Gomez & Laubach 2006 paper, for example, uses outcrop data

to describe fracture aperture size over five orders of magnitude, which seems more than a ‘given scale’.

56 The sentence starts out seemingly about outcrop studies. But the cited reference Makel et al. is a modeling paper; is it really the best call out for the large amount of work that has been done on describing and imaging fractures in outcrop? What about the recent papers by Giovanni Bertotti? See also references in Ukar et al. 2019, Marine & Petroleum Geology, which explicitly covers pitfalls in the uses of outcrop analogs for these purposes and compiles a lot of the relevant literature.

57 I think you ought to add the clause ‘and in horizontal core’. The Hooker et al. 2009 paper focuses on horizontal core. It is also explicitly and example of multi-scale sampling and fracture size analysis.

62 While I agree that there ought to be more multi-scale samplings of fracture attributes, and it’s true that studies of fractures in a narrow scale range are probably more common than ones that look across scales, the way you put it here might make readers think that such studies are rarer than they are. Afterall, the Ortega et al. reference you cite elsewhere is a multi-scale study or fractures and a methods paper on how to conduct such studies and it has 314 citations in google scholar. A review and methods paper that is strangely absent from your list is Marrett, R. 1996. Aggregate properties of fracture populations. Journal of Structural Geology, 18(2-3), 169-178. A more informative accounting here of previous work would be helpful.

67 Do the references explain why these outcrops are viewed as ‘useful analogs’? Or are they about the producing field? I’d prefer to see something more explicit about how you know that these outcrops are valid guides to the specific field in question. Making such a connection is not always straightforward (some common concerns are discussed in Reviews of Geophysics paper mentioned above).

72 But does your assessment include connectivity to fluid flow? The fractures have cement; cement commonly is more pervasive in narrow segments of fractures. The

trace connectivity may well be less than the connectivity to flow. See Olson et al. 2009, AAPG Bulletin.

75 ‘a thin section’ made from ‘samples’; are there more than one of each? Clarify.

76 ‘aperture/fracture width parameter’; The MS would read easier if you would define what you mean by the ‘parameter’ at first use, then stick with it. Are you saying here that you are making no distinction between the ‘aperture’ of an opening mode fracture and the ‘width’ of a fault? For ‘aperture’ do you mean the ‘kinematic aperture’ in the sense of Marrett et al. 1999? Because many opening-mode fractures in the subsurface are sealed, and so are only apertures in that sense. Even the channels in some fracture cements have finite widths (Landry et al. 2016). Ok: I see you define these, at least for opening-mode fractures in outcrop, on 103. But you need to clarify if you are distinguishing between the ‘aperture’ of an opening mode fracture and the ‘width’ of a fault.

77 The usage here (‘whilst…’) is awkward. Rephrase. Does ‘their’ refer to ‘fitting methods’?

80 ‘fracture attribute’ scaling?

89 Spacing data is pretty uninformative, particularly as seems to commonly be the case if fractures are not regularly spaced.Why not go beyond simple spacing with your scanline data. What about the spatial arrangement (such as implement in the Marrett et al. 2018 J. Struct. Geol. approach)? Also note that application of this method to outcrops and subsurface horizontal well data from the same formation and fracture sets has in some instances found differences between outcrop and the subsurface (Li et al. 2018, J. Struct. Geol.) It’s another way to compare outcrops and subsurface.

103 Did you use the Ortega et al. comparator for width measurements?

113 Most of the high-quality ‘size’ data sets that have been published concern 1D aperture size distributions. This is because measuring aperture size on a 1D scanline is

pretty unambiguous. ‘Length’ is another matter entirely. Partly this is due to censoring of long fractures by small outcrops but even knowing what ‘length’ to measure, with segmented, partly disconnected fractures, is a challenge. Olson (2003) makes the case that the length-aperture relations that reflect fracture growth processes may not be the same as interconnected length, which is what’s germane to fluid flow. So I think the generalities in this paragraph need to be treated more carefully. For example, the type of modeling per Olson (2007) (and papers by Michael Welsh) can produce a wide range of types of length distribution in layer-bound systems, and not necessarily log normal. It’s also worth noting that the numerous power law aperture size distributions in sandstone described by Hooker et al. (2014, GSA Bulletin) are almost all from layer-bound opening-mode fractures. The ‘length scale’ defined by layer bound systems for Narr (1991) pertain to the spacing dimension. But fractal clustering (of spacing) has been demonstrated for layer-bound fracture systems (e.g., Marrett et al. 2018, J. Struct. Geol.). So perhaps you should separate generalizations about length, aperture and spacing.

125 This statement makes it seem like the upper and lower limits are unknowable. But sufficiently complete sampling can discern these limits in some cases.

127-132 The ‘state of the art’ for fracture aperture size distributions in sandstone goes bit beyond what is portrayed here. Some fracture size distributions in sandstone are wide and can commonly be described using power laws (e.g., Hooker et al., 2014, GSA Bulletin), but other sandstones have narrow aperture size distributions. There are examples in the literature where wide and narrow size distributions occur in adjacent sandstones subject to the same deformation. The differences appear to correlate with rock composition and the inference is that the differences in pattern reflect at least in part diagenetic effects (and so should be more pronounced in sandstones experiencing deformation at depth). So even if it is ‘generally accepted’ that for many systems power law distributions are useful, in many cases power laws are not an accurate way to describe the fracture population. Wouldn’t it make more sense to say that size distributions ought to be careful measured, but there is no reason to think at this point that power-law scaling is the default setting?

132 What do you mean by ‘over several orders of magnitude at a given scale’? The orders of magnitude are of scale.

151 Are you treating all fractures equally as ‘part of the network’ or is there some parsing of fractures into sets (i.e., the old fashioned way?) per Hancock (1985). The figures look like they record distinguishable sets.

156 The 1D power law size distributions could also be said to be ‘self-similar’. Likewise, the clustering patterns that come out of some 1D spatial arrangement studies (Li et al. 2018, and other papers on faults in the same JSG special issue). Can you expand on the distinction you are drawing here?

157 Of connection types?

165-175 It’s worth keeping in mind, however, that this is the connectivity of traced lines, not the connectivity for fluid flow.

188 Is ‘strictly speaking’ needed?

Moreover, just because the outcrops ‘have long been used’ as analogs for the reservoir, that does not mean that they are good analogs. A straightforward test suggested by Ukar et al. 2019 M & PG is to compare the progress of sandstone diagenesis in the reservoir target and the potential analog. The burial history (and presumably at least elements of the thermal and loading history) of the reservoirs and the outcrops differ (the comparison is between rocks still buried and those at the surface). The comparison of diagenetic state will at least give you ballpark evidence of how similar the rocks are. Opening-mode fracture arrays are typically low strain features that are sensitive to rock properties so it would be easy for analogs to be ‘off’ For example, in the western US Cretaceous sandstone outcrop analogs are commonly poor guides to fractures in the same units in nearby basins as documented in core/well log to outcrop

comparisons (e.g., Li et al., 2018, J. Struct. Geol.). One of the biggest differences is in how fracture size scaling manifests.

If one of the objectives of this paper is to make the case that these outcrops are good analogs, then perhaps this point could be signaled more clearly in the claims in your Introduction.

210 Reason for inference of hydrothermal effects?

233 Have similar features been described from core?

256-261 Sounds interesting. Maybe mention the lengths and numbers of spacings gleaned from these?

279 This point about the sets being ‘active during the same period’ is an interpretation. What observations is it based on? Mutually crosscutting relations? If they are a single episode of mutually orthogonal opening-mode fractures, how does that work with your kinematic interpretation? Sounds like biaxial extension. Clarify.

The support for two or three contemporaneous fracture sets (for example, lines 279-280) seems like a key inference, but where is the description of the evidence that these fractures are contemporaneous? The observation that this inference is based on is not mentioned. Since the fractures are said to be partly calcite filled, do you mean mutually crosscutting or mutually abutting relations? The evidence for this relationship should be described, not just asserted.

291 This sounds like selective sampling. What is the microfracture intensity in the material away from the faults? Such a measurement would be more germane to interpreting the scaling populations of the opening-mode fractures. You would like need multiple contiguous thin sections (like the method described by Gomez and Laubach 2006). Microfractures near the fault does not necessarily mean that there are disseminated microfractures away from the fault.

295-299 This is a pretty short microscanline; cf. Hooker et al. 2009 and 2014 GSA

Bull.

314 Why report spacing when spatial arrangement (e.g. Marrett et al. 2018) is an option? Intensity is only inverse spacing in a meaningful way if the fractures are not clustered, right?

350 Normalization like this is a step advocated by Marrett et al. 1999.

361 Slope of -1?

368 There is something wrong with this sentence.

370 Was ‘too high’?

380-389 How does this compare with the predictions of Olson (2003)? Do you come back to this?

394 No comments on what kind of ‘fractures’ these might be visible in the bathymetry?

412 Corridor-like arrays, in quotes; what are they supposed to signify. Why not at least cite one of the papers that mentions ‘corridors’ like Questiaux et al. and/or a recent review of clustering patterns (Laubach et al. 2018, J. Struct. Geol.). J.M. Questiaux, G.D. Couples, N. Ruby Fractured reservoirs with fracture corridors, Geophys. Prospect., 58 (2010), pp. 279-295. With the scanline data you collected it seems like it would be straightforward for you to quantify the degree and type of clustering.

459-463 Do you say what the physical meaning is of the box counting exponent?

465-467 But does the box counting dimension tell you anything about what that spatial arrangement is like? The patterns qualitatively look clustered, locally at least. Do these box dimension mean the patterns are clustered, and by how much? Is it more clustered than random? Can you test this by comparing your results with 1d coefficient of variation or better, a rigorous method like Marrett et al. 2018, J. Struct. Geol.? It seems as though you collected the 1D scanline data that could go into such an analysis so it would be a quick check. You should also at least consider that possibility that box

counting is returning artifacts.

467 So how do the values obtained from box counting relate to the size distributions obtained from the 1D scanlines?

471-500 The text in this section could stand being broken into smaller paragraphs to help lead the reader through the arguments. There are several separate assertions in there. They don’t seem well supported.

475 Is there independent evidence of sampling bias? Or are you just inferring sampling bias because of the mismatch? I don’t recall you discussing resolution limits or sample sizes with respect to truncation and censoring. Maybe I missed it. It would help if you did, maybe remind the reader here.

481 What about the wide range in aperture and length predictions (orders of magnitude; the grey boxes in 8c)?

481 (fig. 8b) This data ought to be plotted with that of Hooker et al. 2014 compilation, which contains many aperture size data sets, including some from within your general area of interest, and seemingly by the criteria you mention should also match your wide grey bars at least.

Hooker, J.N., Laubach, S.E., and Marrett, R., 2014. A universal power-law scaling exponent for fracture apertures in sandstone. Geological Society of America Bulletin 126(9-10), 1340-1362. doi: 10.1130/B30945.1

Hooker, J.N., Laubach, S.E., Gomez, L., Marrett, R., Eichhubl, P., Diaz-Tushman, K., and Pinzon, E., 2011, Fracture size, frequency, and strain in the Cambrian Eriboll Formation sandstones, NW Scotland. Scottish Journal of Geology, 47/1, 45-56.

483 What do you mean by ‘reduce the influence of an individual data set’? Where do you justify mixing possibly genetically unrelated sets of structures (if that is what you are doing)?

489 Something is missing here. How does a supposed log-normal distribution of faults relate to the reference Olson 2007?

The 2018 J. Structural Geology special issue on spatial arrangement v. 108, Pages 1-290 (March 2018) has several papers that cover fault size scaling and spatial arrangement. These should be consulted. Also, the meaning of 'bed bounded' in discussing faults needs to be defined in the context you are using it. Obviously at some level of consideration most faults are not bed bounded by definition (at least not in the same way opening-mode fractures can be).

496 'this type' of self-similar scaling is vague. The specific regression values? Hooker et al. 2014 reports 3822 fractures from 68 scanlines in eight sandstones having 1D power-law exponents of -0.8 plus or minus 0.1. These results need to be engaged with.

502 I think it's worth mentioning and reminding the reader that this is not the first proposal to measure data at one scale and extrapolate to another. See for example one of the papers you cite where such extrapolations were tested (Hooker et a. 2009). See also Marrett et al. 1999.

512 Where is your comparison with Olson 2003 (and the discussion and replies to that paper)? The reference is in your list. But you only cite it for very general principles (line 59).

515 The 'sublinear scaling' inference is due to Olson (2003) and that reference should be cited here.

520 Why are these basic observations of fracture fills being presented in the Discussion?

522 'apertures filled with fault rocks'?

522 Why do you call these 'hydrothermal' minerals? That implies (at least to some) that they were deposited from a hotter fluid moving through cooler rocks (like 'hydrothermal

dolomite'). Minerals are common in fractures in sandstone (see the 2019 Reviews of Geophysics paper cited elsewhere for a list of examples). Many of these are not 'hydrothermal' in this sense.

523 Do you describe these textures?

534 There is not agreement in the diagenesis community that hydrocarbons necessarily do this. See Bonnell et al. 2006 in Taylor, T. R., Giles, M. R., Hathon, L. A., Diggs, T. N., Braunsdorf, N. R., Birbiglia, G. V., ... & Espejo, I. S. (2010). Sandstone diagenesis and reservoir quality prediction: Models, myths, and reality. AAPG Bulletin, 94(8), 1093-1132.

546 Indeed. Laubach, S.E., Olson, J.E., and Gale, J.F.W., 2004, Are open fractures necessarily aligned with maximum horizontal stress? Earth & Planetary Science Letters, 222/1, 191-195. This papers describes examples of fractures in reservoirs that open despite reservoir conditions, including stress orientations that should have closed them. Bridges are specifically illustrated. Note however, that as this paper states bridges are not needed to keep such fractures open. All that is needed is diagenetic stiffening of the fracture host rock. The calculation illustrating this is in: Olson, J. E., Laubach, S. E., and Lander, R. L., 2007, Combining diagenesis and mechanics to quantify fracture aperture distributions and fracture pattern permeability: In Lonergan, L., Jolley, R.J., Sanderson, D.J., Rawnsley, K., eds., Fractured Reservoirs, Geological Society of London Special Publication 270, 97-112.

548 Or reduce flow to none at all.

550 What you are attempting to do in this section needs to be explained more clearly at the outset. I take it what you are doing is using your scaling data to predict spacing, aperture, and length at a given scale, and testing the efficacy of the prediction by comparing your results to the previously collected spacing data collected by Coney et al. And the length and aperture predictions to some measurements that you made. Is that correct?

551 . . .us to 'illustrate'

553 Reconfigure the sentence to make it clearer that Coney et al. is providing the subsurface data. Why 'systems' and not 'sets'? And do you mean that the 'systems' are spaced apart by the values you quote, or that these are typical spacing of subparallel fractures in a set?

560-561 This seems to say you are using spacing to predict aperture.

562 'more widely spaced faults'?

562 In what sense to faults have 'aperture'? How is fault 'aperture' related to fault 'width'? It seems like the parameter predicted by the scaling would be something more directly related to fault size, like throw, heave, etc. This needs clarification.

567 Was this a slant well through a vertical fracture?

568 By 'core well' do you mean 'cored well'? Was the structure found 'in core' or was it from a cored well, but found on an image log? Your description is ambiguous. Clarification needed.

563-8 This is a section of text where your lack of clear distinctions between opening-mode fractures and faults makes it hard to follow the case you are making. A 14-cm-wide opening-mode fracture is by no means impossible. Excellent outcrop analogs document opening-mode fractures in thick mechanical units of as much as 2 m. And wider calcite-filled veins in reservoir analogs have been described by Hilgers, Urai, and others from Oman. Or is this wide feature part of a fault zone?

574 Why the quotes around "corridor"-like. Are these corridors, or something that only seems like a corridor but isn't? See also comments in line 582. Most usage of this term seems to follow Questiaux et al. 2010. If you are talking about 'interconnected fracture trace patterns' I suggest you use this phrase instead of the ambiguous term 'corridor' which not only has two very different meanings but also, in the sense that you use it, is not justified. Further to this last point, if what you mean is 'corridor' in the

sense interconnected for flow after Manzocchi 2002, you only have information on the connections of the trace patterns, not the flow pathways. Recall the old literature on the Stripa experiment for example.

Questiaux, J.M., Couples, G.D., and Ruby, N., 2010. Fractured reservoirs with fracture corridors. Geophysical Prospecting 58, 279–295.

Manzocchi, T., 2002. The connectivity of two-dimensional networks of spatially correlated fractures. Water Resources Research 38(9), 1162. 10.1029/2000WR000180

577 Maybe 'minimum' from the point of view of physically connected nodes, but it could still overestimate connectivity to flow. You could maybe estimate how reasonable your numbers are by looking at the reported permeability enhancements at Clair (procedure as in Olson et al. 2009, AAPG Bulletin). With the connectivity you report the enhancement should be huge.

584 Where is the analysis of spatial clustering? You have the observations for it, but you portray the 'corridors' qualitatively.

582 From this it seems you are using 'fracture corridors' to mean groups of interconnected fractures. This is confusing usage, especially if it isn't spelled out, since a more common use of 'corridor' is a group of abnormally closely spaced subparallel fractures, i.e., a fracture 'swarm'. These usages are discussed in review in Laubach et al. 2018., J. Struct. Geol. Cluster has been used the way you describe too, and a fairly recent definitions paper quoted that application. But that goes against the bulk of recent usage. In any case, 'corridor' for a tabular feature makes some kind of sense, but 'corridor' for interconnected fault or traces does not. The interconnected traces may not be interconnected for flow (faults or sealed fractures for example) and so may not be 'corridors' for easy fluid flow. I suggest that you spell out what you mean at the outset, then choose something more obviously descriptive as short hand like 'linked traces on fault or fracture map.'

584-587 The expected variability in flow from your networks is a logical jump that doesn't seem well justified. The extent to which the trace connections augment or detract (or do not affect) fluid flow depends on the character of the element. Some faults may be sealing; some numerous and interconnected opening-mode fractures could be sealed. Clusters of 'features' around some faults may be deformation bands that could impede or not affect flow. The flow (if there is any) also depends on the head, if any. This section needs further thought.

589-595 Should this material be in 'Results'?

602 The topics seem to jump around. This text covers spacing (again). Maybe combine all the information for each attribute info in one place.

606 The faults are well connected?

602-612 This paragraph is confusing. You seem to be talking about both 'faults' and 'opening-mode fractures' and 'corridors' (some undefined level of clustering). Or do you mean opening-mode fractures associated with faults?

608 Opening displacements of 10 m? Any bit drop data from Clair to confirm this?

607-608 How do the scanline results show this? Maybe just confusingly put.

614-625 The structure of this paragraphs needs work. It is a mixture of inferences and assertions, but they don't seem to flow one from another.

616 Your discussion or aperture, length, and connectivity should be more nuanced.

616 'aperture' is one element of 'size', so this sentence seems awkward. Also, Philip et al. 2005 SPE REE showed that for fractures in slightly porous rocks length is what matters; aperture size is irrelevant. Cf Long and Witherspoon 1985 on connectivity.

The call out to Odling et al. doesn't seem to fit what you are saying here, which sounds a lot like the parallel plate model. The aperture effect (cubic law) in any case needs to be modified considerably when discussing flow in rocks that have finite host rock per-

meability. Long open fractures can produce considerable permeability enhancement even if completely non interconnected (Philip et al. 2005, SPE REE); and as Philip et al. showed, in those case aperture doesn't matter. Since the sandstone in Clair are porous and permeable apart from the fractures, this is the circumstance likely to apply there.

614-615 The claim of 'usefulness' is I think different from what you've shown. If your claims have been proven, you have shown that your outcrop analogs provide a reliable and perhaps even quantitative view of fracture attributes in Clair field that are otherwise very challenging to measure. Some of these attributes are ones used in reservoir simulation and decision making. If the results are valuable to decision makers, that will depend on the decision making process, the costs to acquire data, whether or not behavior will be changed, etc. I'm just suggesting that you choose your words carefully. Almansour et al., 2020, SPE Re. Eval. Eng. doi: 10.2118/198906-PA has an example of assessing the value of fracture information in an economic/decision making context.

619 The mixing of terms (fault, fracture/opening-mode fracture) and your scaling analysis leads to a confusing claim here. The 'largest fractures' are faults in your accounting, but how do we know they necessarily have any opening displacement associated with them? Some faults are tight and lack opening displacements, and it would certainly be unusual for a fault to have a large opening displacement along its entire length. This needs clarifying.

628 Reference not in list.

634-638 This text sounds more like Introduction. How is it a conclusion of this study?

640-641 This statement about the outcrop being a good analog is framed as a conclusion, but the text seems to merely assert that the outcrops are good analogs. There are some observations about fracture petrology and fracture patterns in the outcrop and in the subsurface, but it didn't seem to me that you built a case for such a definite conclusion. Did you alert readers in your Introduction that this is a claim you are going to make? Or do you want to tone this down and say “based on evidence x, y, and z, the Devonian rocks of the Orcadian Basin in Caithness are plausible analogs for the main reservoir, etc. Based on that inference, we. . .’

652 Isn’t this the first use of ‘vein aperture’? Despite it being a widespread old term, I don’t see any value in retaining it for use in sedimentary rocks. See the short discussion of the term in the 2104 Gale et al. AAPG Bulletin review of fractures in shales. Why not just all these opening-mode fractures and specify the mineral content?

673 Introducing conclusions about drilling strategy in the Conclusions? Did I miss the discussion of this topic?

The very broad range of aperture and length predictions, for example in figure 8, ought to be discussed where you make claims and conclusions about how useful your findings are for practical application. Are aperture and length predictions that are within two orders of magnitude likely to be practically useful? How can you demonstrate this?

And why not compare to the claims and the uncertainty ranges in the Hooker et al. 2009 across-scales predictions?

Figure captions

872 Is the box counting method detecting artifacts?

876-878 Is this much information on the standard Terzaghi correction really needed?

876 Is this real data, or are these example distributions?

896 To make these figure captions more stand alone and clear, the ‘where’ and ‘what’ information on the various scales (a-i) and orientation patterns should be stated in the figure caption. Are these observations all from the target sandstone?

902 ‘general influence of present day stress’ is vague.

909 ‘aperture and vein width’?

Figures

The figures numbers and quality are good. As noted above it would be advantageous to graphically compare results with those of Hooker et al. 2014.

Figure 2. The height categories Random, Strata bound, and Non-stratabound don’t cover the most common subsurface fracture height pattern as documented in fractured sandstone core; that height pattern was called ‘top bounded’ by Hooker et al. (2013, J. Struct. Geol.). The classification proposed by Hooker et al. (2013) has proven to be useful; it’s also replicated in Gale et al. (2014) shale fracture review paper. I recommend that you apply it.

Fig. 3, ‘f’ lacks a label; both ‘e’ and ‘f’ need graphic bar scales. The inset in ‘e’ and ‘f’ are both quite small and hard to read. From the figure caption it’s hard to tell what is meant to be portrayed in e and f. The photomicrograph appears to show blocky, twinned calcite (is this inside a vein?) possibly containing a fracture (‘fr’).

Fig. 6 Why not graph the micro and macro data on the same plot? Ok; I see you have this in figure 8. How do the aperture size distributions compare with the values reported by Hooker et al. (2009; 2014)? These figures out to be arranged such that the slopes are not distorted by having different scales.

Fig. 8 The aperture size data should at least in the text be compared and contrasted with the compilations in Hooker et al. (2009; 2014).

Fig. 9 The significant figures on the regression look too high. Check.

Interactive comment on Solid Earth Discuss., https://doi.org/10.5194/se-2020-15, 2020.